

# REPLY TO COMMENT ON “SPECTRA OF STRONG MAGNETOHYDRODYNAMIC TURBULENCE FROM HIGH-RESOLUTION SIMULATIONS”

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## ABSTRACT

In a Comment by Perez *et al.* (2014) it is claimed that recently published simulations of Beresnyak (2014) are grossly underresolved, compared to theirs, and that Beresnyak (2014) failed to estimate numerical error. Both claims are contrary to the fact. Firstly, as far as numerical resolution is concerned, Beresnyak (2014) was using  $k_{\max}\eta_{41}=1.06$  resolution criterion, while Perez *et al.* has been using  $k_{\max}\eta_{41}=0.8$ . Obviously,  $1.06 > 0.8$ . Secondly, Beresnyak (2014) have estimated numerical error and found it to be below  $3 \times 10^{-3}$ , which is properly explained in the paper. On close inspection of the Comment I have not found a single numerical value or parameter pertaining to the criticized paper (Beresnyak 2014), and it is completely unclear how the Authors came to their conclusions.

Numerical simulations of MHD turbulence in strong mean field with pseudospectral code date back to early 2000s (Cho and Vishniac 2000). Recently there has been a debate regarding the spectral slope in high-resolution simulations between J. Perez *et al.* and A. Beresnyak. Perez *et al.* has been repeatedly bringing numerical inaccuracies as the main source of disagreement (see, e.g., Perez *et al.* 2012, 2014). The latest Comment (Perez *et al.* 2014) describe Beresnyak’s simulations as “drastically unresolved” and their own simulations as numerically accurate. This is quite surprising, considering that Beresnyak’s simulations are, in fact, better resolved than those reported by Perez *et al.*

While carefully reading the Comment I have found no parameters from the criticized paper, Beresnyak (2014). The Author’s claim seems to be completely arbitrary. They are trying to allege that some of their own grossly underresolved simulations presented on their Figs. 1 and 2 have anything to do with Beresnyak (2014). This is simply not the case.

The main resolution criterion in turbulence simulations is based on a ratio of Kolmogorov (dissipation) scale  $\eta$  to the grid scale. In pseudospectral simulations of both groups the box size is  $2\pi$  and the grid size is  $2\pi/N$ , where  $N$  is the mesh size. Another useful quantity is a maximum wavenumber  $k_{\max}$ , equal to  $N/3$  in the 2/3 dealiased simulations of both groups. It follows that  $k_{\max} = 2\pi/(3\Delta)$  and both the ratio  $\eta/\Delta$  and the product of  $k_{\max}\eta$  can be used as a numerical resolution parameter, with higher parameter corresponding to higher numerical accuracy.

The Kolmogorov scale  $\eta$  is itself a function of the model. This, however, is not a problem, as long as the same definition is used for comparison. Perez *et al.*

(2012) designate  $\eta_{41} = (\nu^3/\epsilon)^{1/4}$ , which is the classic Kolmogorov scale and their Fig. 9 indicate that  $k_{\max}\eta_{41} = 0.8$  in their case. This can be independently verified by using parameter  $\nu = 1/\text{Re}$  from Table I, simulations RB1a, RB2a and RB3a and parameter  $\epsilon = 0.15$  from page 8. Beresnyak (2014), however, uses  $k_{\max}\eta_{41} = 1.06$ , which is a better resolution that corresponds to higher numerical accuracy. How the Authors of the Comment concluded that they have better resolution is totally puzzling. They do not mention the resolution of Beresnyak (2014) for that matter.

The slightly reduced numerical accuracy of Perez *et al.* (2012) is not the biggest problem of their paper, however. As we noted earlier in Beresnyak (2013), this paper have severe methodological flaw of claiming correspondence between theory and strange numerical “measurement”. On Fig. 8 of Perez *et al.* (2012) it is claimed that the measured length of the inertial range follows scaling from Boldyrev (2006), namely  $Re^{2/3}$ . On close inspection, however, it is evident that the Authors calculated “datapoints” by the formula  $0.025\epsilon^{2/9}\Lambda^{-1/9}\nu^{-2/3}$ , where  $Re = 1/\nu$  and 0.025 is a number, arbitrarily chosen by the Authors. Using  $\epsilon$  and  $\Lambda$  quoted in Perez *et al.* (2012) the product  $\epsilon^{2/9}\Lambda^{-1/9}$  can be approximated as 0.517, after which the dependence  $0.0129Re^{2/3}$  reproduces the Author’s plot on the bottom of Fig. 8. Some time have passed after publication of Beresnyak (2013), but the claim from Perez *et al.* (2012) have not been recalled by the Authors yet, which is deeply troubling, in my opinion.

In the end of their Comment, the Authors claimed that Beresnyak (2014) have failed to perform numerical convergence study and estimate numerical error. This is contrary to the fact, see Beresnyak (2014), page 2.

## REFERENCES

- A. Beresnyak, ApJ **784**, L20 (2014).
- J. Cho and E. T. Vishniac, ApJ **538**, 217 (2000),  
arXiv:astro-ph/0003404.
- J. C. Perez, J. Mason, S. Boldyrev, and F. Cattaneo,  
Physical Review X **2**, 041005 (2012),  
arXiv:1209.2011 [astro-ph.SR].
- J. C. Perez, J. Mason, S. Boldyrev, and F. Cattaneo, ArXiv  
e-prints (2014), arXiv:1409.8106 [astro-ph.SR].
- A. Beresnyak, ArXiv e-prints (2013),  
arXiv:1301.7425 [astro-ph.GA].
- S. Boldyrev, Phys. Rev. Lett. **96**, 115002 (2006)